

Interactive comment on “Aerosol trends as a potential driver of regional climate in the central United States: Evidence from observations” by Daniel H. Cusworth et al.

Anonymous Referee #2

Received and published: 19 April 2017

This study explores the connection between changes in regional tropospheric aerosol burden and changes in meteorological parameters measured at two locations in the eastern U.S. – Bondville and vicinity, and Goodwin Creek (with greater emphasis on the former location). The study builds upon previous analyses of the SURFRAD data at these sites by (1) incorporating additional measurements from USCRN, CIES, ICN, Ameriflux, and (2) examining the impact of changes in radiation on surface temperature and soil moisture. As such the results of the analysis are interesting and confirm the complex interactions between aerosols, clouds and radiation. However, in my assessment the usefulness of the manuscript can be improved through additional substantiation of some of the conclusions, and improvements in some aspects of the discussions.

[Printer-friendly version](#)

[Discussion paper](#)



The following suggestions are offered:

1) The goals and objectives of the study should be stated more clearly and supported through the manuscript discussions. The introduction (line 4-5 page 4) suggests that the study goal is to investigate the role of aerosols on the “warming-hole” reversal and to reconcile the conflicting hypothesis on this reversal. One would infer that the driver for this would be the impact of changing aerosol burden on radiation (either through direct or indirect effects). The analysis examines trends in SWdn and its association with trends in aerosols, and attempts to link these with trends in radiation and soil moisture. Yet, the analysis concludes (page 12, line 15) that though decreasing AOD may have contributed to all-sky SWdn trends, diagnosing the causes of the trends is beyond the scope of this paper, which contradicts the objective of reconciling existing conflicting hypothesis was met. There is a lot of good analyses that gets lost in between - perhaps clearly identifying what aspects of the trends, the analysis supports, would help convey a more compelling story.

2) Can Bondville and nearby sites be considered representative of the region where the “warming-hole” has been identified to occur? Some discussion on the suitability of the site for the analysis should be discussed in section 2, especially since the analysis at Goodwin Creek is limited.

3) The analysis attempts to examine the unexpected trends in clear-sky diffuse radiation identified in previous studies (e.g., Gan et al.). While this is a noteworthy attempt, the description of the numerical experiments would benefit from additional details. Specifically, some discussion on how well the CERES retrievals represent the cirrus cloud fraction would be useful to the readers? At locations where the cirrus cloud fractions show strong increasing trends (Figure 3), what trend does the RRTM estimated diffuse radiation show? What may be the likely reasons for the increase in measured diffuse SWdn at Bondville? Also does the CERES cirrus cloud fraction adequately represent aircraft contrails and trends?

[Printer-friendly version](#)[Discussion paper](#)

4) The RRTMG calculations conducted for the current study should be described in more detail. In particular, what was the temporal resolution of the data input to RRTMG? The discussion on page 6 (lines 29-30) suggest that the AOD and CERES information used in the calculations are monthly means. If finer temporal resolution was used for this data would the results be different? I believe the SURFRAD data is available at finer temporal resolution, but perhaps the CERES is not – some discussion of these aspects of the calculations would be useful in putting the results in context.

5) The purpose (and conclusions) of the MLR analysis in section 4 are not readily apparent. Some discussion on why the MLR captures only 20-26% of the SWdn variability and likely factors influencing the rest would be useful. If increase in clear-sky SWdn (Figure 2) and decrease in AOD (Figure 1) are observed at both Bondville and Goodwin Creek, why does the MLR for Goodwin Creek not capture any of the variability with respect to AOD?

6) Page 9, line 24-25: what is the connection between the R2 in Table 1 and those from the correlation between model and observed SWdn? What does the suggested similarity in these R2 imply?

7) Page 9, line 32: The terminology “fine aerosol regimes” is ambiguous - regimes based on burden, composition, or size?

8) Page 10, lines 16-17: the magnitude of the difference in SWdn between sunny and non-sunny summers is compared with magnitude of 2000-2014 change – It is not apparent to me what significance one can draw from this comparison or what it tells us about the variability in SWdn.

9) Similarly, the significance of the magnitude of change in SWdn between PER1 and PER2, relative to the 2000-2004 change is not obvious?

10) Page 10, line 24: cloudy and relatively cooler conditions across the eastern U.S. for the summer of 2004 are well-documented – it is thus curious why summer 2004 is

[Printer-friendly version](#)[Discussion paper](#)

classified as sunny?

11) Page 12, line 25: should probably acknowledge that the increase in diffuse component of clear-sky SWdn follows conclusions of earlier studies analyzing SURFRAD measurements.

12) Page 13, line 17-19: This discussion is somewhat vague as it does not state which conclusions of the Mickley et al (2012) study are supported by the current analysis. The sentence should be reworded to clearly state so.

13) Page 13, lines 23-29: It is not clear what to conclude from this discussion which starts by saying that aerosols play a “small but significant” role in regional meteorology (based on Bondville data), then casts doubt on these aerosol-radiation interactions based on analysis of data at Goodwin Creek, but then ends by saying that these interactions could be potentially important. Perhaps aspects of direct and indirect aerosol radiation effects are being mixed and should be explicitly stated. This discussion could be expanded to add clarity.

14) Page 14: line 6: The conclusion that high loading of anthropogenic aerosols during the 1970s contributed to moist conditions, countering the SST influence and reducing drought risk, is somewhat speculative given that no aerosol loading or meteorological parameters from that period are analyzed. Much of the analysis focuses on data from 2000-2014. The extrapolation of results to another period and broader domain should be explained in more detail. In its current form the suggestion is not very convincing.

15) Section 6 would benefit from brief discussion on limitations of the current analysis. While I acknowledge the possible influence of aerosol radiation interactions on precipitation and hydrological cycles, inferences on drought especially in other regions, should be cautiously drawn. If the associations between aerosol burden and radiation are site dependent (as conveyed by the associations at Bondville and Goodwin Creek), what inferences can one draw on the robustness of the association between AOD and soil moisture essentially drawn from data at Bondville (and vicinity sites)? Caveats for

extrapolation of the inferences to other geographic regions should be provided.

16) Page 5, line 26: “correspond those” should be “correspond to those”.

Interactive comment on Atmos. Chem. Phys. Discuss., doi:10.5194/acp-2017-208, 2017.

ACPD

Interactive
comment

Printer-friendly version

Discussion paper

